The misdirected quest

ARTICLE

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Peter Lamont on how early psychologists turned to the grand wizards in an effort to transform illusions into a reality

Psychologists are supposed to be experts on how people think and behave. Yet magicians have always displayed a more wonderful ability to direct thoughts and actions. Thus, every now and again, for well over a century, psychologists have tried to understand how magicians do it.

So, in their magical quest to discover the secret reality behind the illusion, how have psychologists done?

question

What, if anything, can psychologists learn from magic?

resources

Lamont, P., Henderson, J.M. & Smith, T. (2010). Where science and magic meet: The illusion of a 'science of magic'. *Review of General Psychology, 14*(1), 16–21.

At the end of the 19th century, Hermann and Kellar were the two greatest conjurors in the world, though who was greatest depended upon whose publicity one believed. In the United States they competed over audiences and advertising space, and each considered the other his arch-rival. When Hermann died in 1896, Kellar was free to establish his reign and, aside from his notable achievements in the world of magic, he was almost certainly the inspiration for the Wizard of Oz. But before Kellar became the grand wizard, and shortly before Hermann's death, the two great rivals agreed to compete in a quite different environment – the psychological laboratory.

This was not the first time psychologists had taken an interest in conjuring. Gustav Fechner had observed a spiritualist medium in 1878, and he had concluded that, if it was not trickery, then it was proof of a fourth dimension in space. Wilhelm Wundt, who had observed the same medium, had simply dismissed it as conjuring, though he had no idea how it was done (Marshall & Wendt, 1980). Meanwhile, in Britain, William Benjamin Carpenter had been relying upon the writings of conjurors in an attempt to explain why tables were floating in Victorian drawing rooms (Carpenter, 1871). Thus, psychological interest in conjuring was provoked by the need to distinguish between miracles and magic, and to frame the extraordinary as nothing more than curious. The curiosity, however, had continued.

In 1893 Alfred Binet had invited five of France's most eminent conjurors to his laboratory in Paris. Binet had presented an account of how magic worked, based on the writings of conjurors, and observed some similarities to certain contemporary psychological theories. Following James Sully's distinction between active illusions (such as hallucinations) and passive illusions (that were universally experienced), Binet had placed conjuring effects into the latter category and argued there were positive illusions (seeing what is not there) and negative illusions (not seeing what is there). Having observed some conjuring tricks, and with reference to recent experiments on letter recognition times, he had used new chronophotographic apparatus to allow him to view some basic sleight of hand tricks slowed down. In doing so, and by removing the conjurors' commentary, he had found that the illusion was destroyed. This, for Binet, had been a successful separation of brute sensation from mental interpretation (Binet, 1894). As it happens, Georges Melies, one of the conjurors present, would make practical use of this distinction shortly afterwards when he invented the first special effects in early cinema. Binet, of course, moved on to other topics.

The man who brought Hermann and Kellar together, however, was Joseph Jastrow, who had recently established a psychology department at the University of Wisconsin. He was interested in a wide range of psychological topics, including perception, and was the first psychologist to use the duck–rabbit illusion in a psychology article. Now he sought to make a similarly difficult distinction between two quite different beasts. His reason for conducting these curious experiments was, he claimed, that 'the influence of special kinds of occupation and training upon the delicacy, range and quickness of sensory, motor and mental powers is an important and interesting problem'. For this reason, 'psychological tests made upon virtuosi are desirable, even if in individual cases they suggest no very decided conclusions'. He therefore employed a range of

references

Bernhard, R. (1936). *The psychology of conjuring*. Stanford: Author.

Binet, A. (1894). Psychology of prestidigitation. *Annual Report of the Board of Regents of the Smithsonian Institution* (pp.555–571). Washington, DC: Government Printing Office.

Carpenter, W. (1871). Spiritualism and its recent converts. *Quarterly Review, 131*, 301–353.

Coon, D. (1992). Testing the limits of sense and science: American experimental psychologists combat spiritualism. *American Psychologist, 47*(2), 143–151.

Dessoir, M. (1893).The psychology of legerdemain. *The Open Court, 12*, 3599–3606.

Jastrow, J. (1896). Psychological notes upon sleight-of-hand experts, *Science, 3*, 1896, 685–689.

Kelley, H. (1977). *Magic tricks: The management of causal attributions. Perspectives on attribution research and theory: The Bielefeld symposium*. Cambridge: Balliger.

Kuhn, G. & Land, M.F. (2006). There's more to magic than meets the eye. *Current Biology, 16*(22), 950–951.

Lamont, P. & Wiseman, R. (1999). *Magic in theory: An introduction to the theoretical and psychological elements of conjuring*. Hatfield: University of Hertfordshire Press.

Lamont, P., Henderson, J.M. & Smith, T. (2010). Where science and magic meet: The illusion of a 'science of magic'. *Review of General Psychology, 14*(1), 16–21.

Macknick, S. & Martinez-Conde, S. (2009). Real magic: Future studies of magic should be grounded in neuroscience, *Nature Reviews Neuroscience, 10*, 241.

Marshall, M.E. & Wendt, R. (1980).

psychophysical tests that he thought 'to be related to the processes upon which their dexterity depends' and which he felt 'most likely to yield definite results' (Jastrow, 1896, p.685). As it turned out, he was wrong on both counts.

Jastrow compared the two wizards in terms of tactile sensibility, such as point discrimination (both were below average), weight discrimination (both were below average), and length discrimination by touch (Kellar was below average, and Hermann average). In tests of visual perception, they were unable to divide lines equally or judge lengths any better than others, and in a test using the 'form alphabet' (in which the subject had 90 seconds to identify as many instances of a chosen symbol in a long string of symbols), Kellar was average and Hermann 'did not fully comprehend what was wanted'. Both did manage to excel in rapidity of movement of finger and forearm, and in reaction time to visual and tactile stimuli. However, when the response involved some kind of discrimination, they were again below average.

Jastrow conceded that the positive results were 'small', that 'any suggestions which the data seem to warrant must be put forward with great caution', and that the methods were better adapted to statistical groups than individuals. So it was that he pioneered a trail for psychologists to study the topic and reach, to use his phrase, 'no very decided conclusions' (pp.686–689).

Just a few years later, Norman Triplett wrote a thesis, suggested by G. Stanley Hall, and published it in the *American Journal of Psychology* in 1900. This substantial article discusses the origins of deception in mimicry, and links various themes in conjuring to contemporary associationist psychology. Like Jastrow and Binet, Triplett (1900) relied upon the writings of conjurors in order to understand how conjuring worked, though he did conduct an experiment using a vanishing tennis ball. It seems to have been prompted by an experiment on suggestibility, carried out by Binet. In that experiment, subjects had repeatedly walked towards a ball hanging in front of a black background, reported when they could see it, and their distance from the ball had been noted. They were then asked to walk forward again but this time the ball was not visible; nevertheless, when they got to the same point, subjects reported seeing the ball. This, of course, had been part of Binet's work on hypnosis.

Kellar, levitating

Triplett's experiment, however, was based on an illusion sometimes used by conjurors. A demonstrator, who was sitting behind a desk in a schoolroom, threw a tennis ball (though he found it worked equally well with an apple or a silver dollar) about three feet in the air and caught it. He threw it a second time, slightly higher, and caught it again. He then secretly dropped the ball on his lap and mimed throwing it a third time. When asked what happened, nearly half the pupils (40 per cent of the boys and 60 per cent of the girls) said they saw the ball go up and disappear. On the gender difference, Triplett observed that '[according to Havelock Ellis] ecstacy, trance, seeing of visions, illusions of fancy and tendency to hallucinations, are more frequent in females. Pliny tells us that women are the best subjects for magical experiments, and Bodin estimated the proportion of witches to wizards at not less than fifty to one'. On the efficacy of the illusion, however, Triplett concluded that 'these cases of suggestions of repetition cited, both from the laboratory and the stage, show plainly that the conjurer's maxim "to first really do what you would have the audience believe you do" rests upon a physiological basis' (pp.491–494).

Triplett's conclusion might suggest the possibility of a scientific theory of magic, but the 'maxim' he cites, like all so-called 'rules' in magic, is little more than a common theme. There are no universal rules in magic; conjurors regularly 'tell the audience what they are going to do' and frequently 'repeat a trick'. One of the reasons there has never been a scientific theory of magic is the problem of identifying general rules in a form of interaction that is specifically designed to circumvent what rules are thought to apply in a given situation. For every example in which a conjuror 'really does' what will then be simulated, there are countless examples of a conjuror simply doing something devious once and it being regarded as innocent. Such actions are regarded as innocent because they are deemed natural (i.e. not suspicious) in that time and place, and so are not noticed (or are noticed but then promptly forgotten). Whether it is necessary to 'condition' one's audience to an action depends entirely upon whether that particular audience at that time might regard that particular action as suspicious. The highly contingent nature of deceptive entertainment is one reason why, despite superficial appearances, there has never been a 'psychology of magic' in any meaningful sense (Lamont et al., 2010).

The fact that Jastrow, Binet and Triplett were writing on the topic around the same time might suggest that this was a subdiscipline of psychology (e.g Coon, 1992), but this was an illusion. These were ad hoc publications, in a variety of academic and non-academic journals, and the lack of coherence was such that Triplett was not even aware that Jastrow had recently written on the topic. Indeed, Triplett's ball experiment was not replicated until more than a century later, when Kuhn and Land (2006) employed modern eye-tracking equipment to provide a rare example of psychology providing some insight into why a particular magic

Wilhelm Wundt, spiritism and the assumptions of science. In W.G. Bringmann & R.D. Tweney (Eds.) *Wundt studies: A centennial collection*. [pp.158–175]. Toronto: C.J. Hogrefe.

Nardi, P. (1984). Towards a social psychology of entertainment magic. *Symbolic Interaction, 7*, 25–42.

Triplett, N. (1900). The psychology of conjuring deceptions. *American Journal of Psychology, 11*(4), 439–510.

trick works. But the attempt to identify general rules in magic that might form the basis for a scientific theory has been in vain. When Max Dessoir, the German philosopher and psychologist, claimed that uneducated people were harder to fool (Dessoir, 1893), Triplett claimed the opposite. Neither carried out any experiments on this, but there would have been little point. Magicians can make a living from fooling children or adults (including not only academics but also fellow magicians) by employing similar methods or misdirection techniques in slightly different ways.

Thus, for over a century, psychologists have periodically wondered why magic has been neglected, and offered various theoretical frameworks, none of which is self-evidently better than the last (e.g. Bernhard, 1936; Kelley, 1977; Nardi, 1984; Lamont & Wiseman, 1999). The most recent calls for a scientific theory are simply the latest in a long line of attempts to cut the conjuring cake; the slices are fresher, but they are no more representative. This is not to say that magic cannot provide insight into psychological processes, but to do so requires (and has always required) focused application of specific conjuring knowledge, and a bit of historical knowledge might not go amiss either. Indeed, some awareness of history might have curbed the enthusiasm of those who have called for a scientific theory of magic, or have proclaimed (on the basis of no experimental work whatsoever) that 'future studies of magic should be grounded in neuroscience' (Macknick & Matrinez-Conde, 2009).

More generally, the history of psychological interest in magic, though it has not been significant in terms of findings, nevertheless does tell us something about the how and why of psychological knowledge. For one thing, we can see why some people choose to take an interest in particular topics. After all, most psychologists who have written on the topic have had a personal background in magic, and many have been in the business of debunking psychic phenomena. Just as Carpenter often cited the writings of conjurors in his anti-spiritualist articles, Jastrow's attempt to understand conjuring was directly linked to his desire to debunk mediums, and several similarly sceptical psychologists have written on the topic since, a reminder that the production of psychological knowledge is invariably shaped by wider concerns.

What history also shows is that experimental methods do not compensate for a superficial understanding of an enormous range of situational practices, and that even the most up-to-date technology does not necessarily provide better knowledge in matters of complex human interaction. After all, so far as psychology is about understanding and predicting human behaviour, the experienced magician can demonstrate his or her ability to do so regularly (with a replication rate most psychologists would envy), and to do so in the real world. Magicians have been able to do this for centuries, but how they have done this has changed according to the way in which people think and behave at different times and in different places. Psychologists can indeed learn from magic, but it is magicians who understand the real secrets.

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